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virtue of its structure even if it were unconscious."

I have much respect for Professor Brooks' abilities and work as a biologist, but in the above sentences he commits the common error of confounding volition with consciousness in a way which will surprise any student of mental phenomena. I am not aware that any well-read person in modern times has proposed the hypothesis that 'volition,' or doing 'as it likes,' is a property of the vast majority of protoplasm, while every naturalist knows that consciousness is a property of protoplasm, though not of all protoplasm so far as our means of observation permit us to judge. Students of cells and tissues are very frequently not students of consciousness, and I will therefore add another commonplace of psychology, and that is that the responses of conscious protoplasm to stimuli are as strictly regulated by necessity as the responses of unconscious protoplasm, though the necessity is of a different kind.

The proposition that a muscular contraction is influenced, *i. e.*, directed by a conscious state, may be a matter of mere opinion, or it may be a working hypothesis, or it may represent a fact. Mankind generally, including many scientific men, hold it to be a fact. Lord Kelvin, according to Prof. Gage, is of this number, though he calls it a 'miracle.' However, Prof. Brooks will probably allow that it is a permissible working hypothesis, although he does not say so directly. If we grant that it is true of man, which most of us do, no one has yet shown where the line is to be drawn, as we descend the scale of animal life, at which sensation ends. In fact, centers of special sense are alleged to exist in many Protozoa, and if special sensation exists it is probable that general sensation exists still lower down in the scale.

As to whether such sensation, if it exists, has any effect on structure, the reasons for thinking that this occurs through the medium of movements have been stated so often that it is not necessary to repeat them here. I only refer for a resumé of some of the evidence to a book by myself which will probably be issued by the Open Court Publishing Co. by the beginning of next month.

A common source of obscure thinking among

naturalists is the assumption that reflex and automatic acts disprove the agency of conscious states in the direction of movements. Evolutionists, however, look for the origin of things, and some of them find consciousness, as a cause of the direction of new movements now, to be an equally supposable cause of new movements at former periods of the earth's history. Here we have again a legitimate working hypothesis; although it is not necessary to account for all the movements of organic matter.

Of course, the opposing view to the hypotheses above mentioned involves the assumption of their falsity. To give the opposite position the standing in court adopted by Professor Brooks, I quote him with variations, as follows: "If the learned bodies which give their allegiance to the utterances I have quoted will publish the evidence that consciousness and volition can" influence Professor Brooks when he writes a learned article, or makes an address on a biological subject, "they will not only demonstrate their own scientific eminence, but by settling a question which has never ceased to vex the mind of man they will make the closing years of the nineteenth century memorable for all time," etc. Thinkers will adopt one or the other of these hypotheses as they see fit, but when they touch the metaphysical side of the question they must give to it that attention which it deserves.

Professor Brooks' plea for suspense of judgment is wise. But the formulation of a hypothesis need not alarm him. Builders generally know the difference between the scaffolding and the building. And a builder will value the indication of faults in his scaffolding rather than general disquisitions on the uselessness of scaffolds in general.

E. D. COPE.

P. S. I hope to make shortly some comments in the pages of the American Naturalist on previous articles in SCIENCE by Profs. Baldwin and Cattell.

ABSORPTION OF TERRESTRIAL RADIATIONS BY THE ATMOSPHERE.

I AM certainly glad that Prof. Davis (SCIENCE p. 485, Oct. 11, 1895) objected to the extreme terms which I used in referring to the blanket-effect of our atmosphere. I object to them

myself, and must have used them in a moment of mental aberration. I should have said that the bolometer had given us most of the reliable data concerning the absorption and transmission of radiant energy by the atmosphere, although at that time I fully believed, both from a general knowledge of Prof. Langley's work, and from conversations with him, that the atmosphere was a pretty good valve. Prof. Davis's references and a recent study of the published data show that the valve is leaky indeed. Still, if the atmosphere absorbs 50% of the Sun's radiations, and 50% of those from the earth, we have 25% of the Sun's radiations let in and not let out. If we take the figures which I believe Langley recommends, 70% for the solar, and 40% for the terrestrial radiations, we should have a catch of 40% of that originally arriving from the Sun.

Many unexplained points concerning this complex problem continually appear. What becomes of the 30-40% of the solar radiations and the 40% of terrestrial radiations absorbed by the atmosphere? It has but little mass and low specific heat, and yet it does not get hot, except in its lower layers. This source of energy it seems to me would be more than sufficient for all meteorological phenomena. Prof. Langley's data, voluminous and wonderful as they are, still appear incomplete in certain very important directions, leaving a very attractive field for investigation.

As to terminology, it seems to me very convenient to speak of 'heat rays' so long as we know exactly what we mean by the expression. We are all familiar with 'light rays,' and a 'heat ray' is the same thing, only, as Maxwell says, considered in its 'thermal aspect.' The term 'ray' is no doubt bad, but it is convenient and should be permissible with a tacit understanding that it is only a makeshift term. It would, of course, be better if we had some term to signify energy in its radiant form, as to direction of propagation, wave front, etc., but so long as we have not, and inasmuch as we all recognize its identity, why not use the old names and avoid multiplication of words. Even Prof. Langley's 'Luminous heat' ought to mislead no one; evidently he refers to the heat effects of that kind of radiant energy which is also capable of

producing light effects; 'dark heat rays' are incapable of so doing. When Professor Langley speaks of the 'radically different character of the heat in two maxima' he refers, of course, to their different wave-lengths. A similar remark about a treble and bass note would not mislead any one into the idea that both were not sound. I fail to see what is wrong with the last quotation from my article, or exactly what is meant by the 'mis-recognition of the early part of this century.'

I sympathize most sincerely with Professor Davis in his demand for precise terminology, but we must not allow even this worthy desire to lead us into complexities of expression which may be even more fatal to perspicuity than old terms with modern significations.

W. HALLOCK.

COLUMBIA COLLEGE, October 11, 1895.

A REPLY.

EDITOR OF SCIENCE: If it be fair to presume, as does Dr. Emory McClintock on page 453-4 of SCIENCE, under a heading which I think should be 'Professor Halsted Corroborated,' that because neither in a private letter nor in print one specifies his many mistakes, therefore one did not disapprove both his 'half on Saccheri as well as the half on Gauss,' then I must beg of SCIENCE a line to say that among other mistakes in this letter of his, he is completely wrong in saying of me: "He found that the two words *diuturnum prælum* were meant by Saccheri to indicate a mental attitude of constant war against the 'hypothesis' as heretical."

GEORGE BRUCE HALSTED.

AUSTIN, TEXAS, October 7, 1895.

THE RUDOLF LEUCKART CELEBRATION.

SEVERAL months ago the following circular (Cf. SCIENCE, Vol. I., p. 187) was sent out from Leipzig, signed by about a hundred and fifty scientists from various parts of the world:

"Zur Feier des am 13 December, 1895, stattfindenden fünfzigjährigen Doctorjubiläums von Rudolf Leuckart, dem Nestor unter den deutschen Zoologen, dessen Wirken weit über den Kreis seiner Specialwissenschaft hinausreicht, fordern die ergebenst Unterzeichneten zu Beiträgen auf. Im Herzen seiner zahlreichen